is certainly one of the most delightful and attractive countries that I have ever struck in any part of the world.

Professor Gregory also told us something about the internal discomfort—I forget exactly what the cause was—which he and his party suffered, or were threatened with suffering, in one part of the journey. That recalled a story of which I myself was the central figure in that very part of the world. I had just eaten breakfast one morning when my Chinese interpreter came to me and said, " Sir, may I speak ? " I said, " Yes, Mr. Chu, you may speak." " Sir, I hear very funny story." " Yes, Mr. Chu, let's have it." He went on, " Sir, they say that the people who eat eggs of this village suffer very terrible pain and sometimes die." I replied, " Mr. Chu, that would be a funnier story if it was not for the fact that I have just eaten three eggs myself ! " I am happy to say that the prophecies of ill effect from the eggs of that particular village were not, at any rate in my case, fulfilled. These stories probably arise fron the unhealthiness of some of the valley bottoms where fever, particularly malarial fever, is extremely prevalent. In fact, as we know from the stories told by Marco Polo himself in his great journey across Asia, many of these valleys were said, in those days at any rate, to be impassable by man for that very reason.

Nothing remains now except for me to express, on your behalf, your appreciation of Professor Gregory's admirable paper and of the most excellent postscript which was added to it by his son.

WEGENER'S HYPOTHESIS OF CONTINENTAL DRIFT

Philip Lake

Read at the Afternoon Meeting of the Society 22 *January* 1923.

WEGENER'S views are now so widely known that a very brief introduction will be sufficient. He imagines that the continental masses are patches of lighter rock floating and moving in a layer of denser rock, of unknown thickness ; and this denser rock forms the floor of the oceans. Following, with a slight alteration, the terminology of Suess he calls the lighter material the Sial and the denser layer the Sima. Suess's words are Sal and Sima, and there is no advantage in the change. Suess thinks, however, that the Sal is continuous, covering the globe completely, and this is a fundamental difference.

Wegener does not suppose the Sima to be actually liquid, but he believes it to be plastic enough to yield slowly under the strains to which it is subjected, much as a stick of sealing-wax supported at its ends will gradually bend without ever losing its apparent rigidity.

In this paper I shall not discuss the possibility of Wegener's conception. He does not profess to explain completely why the continents should move, but he claims to have proved conclusively that such movement has taken place. It is the evidence on which he relies, and more particularly the geological evidence, that I propose to examine.

One of the arguments on which he lays great stress is derived from the relative frequency of different heights and depths upon the Earth. In the diagram which he gives (Fig. 1) the frequencies are expressed in percentages of the total area of the Earth's surface and have reference to a vertical interval of 100 metres. Thus, for example, the frequency assigned to the altitude of 1000 metres is the percentage of the Earth's surface occupied by land between the contours of 950 metres and 1050 metres. The diagram shows two well-marked maxima, one at about 100 metres above sea-level and the other about 4700 metres below it.

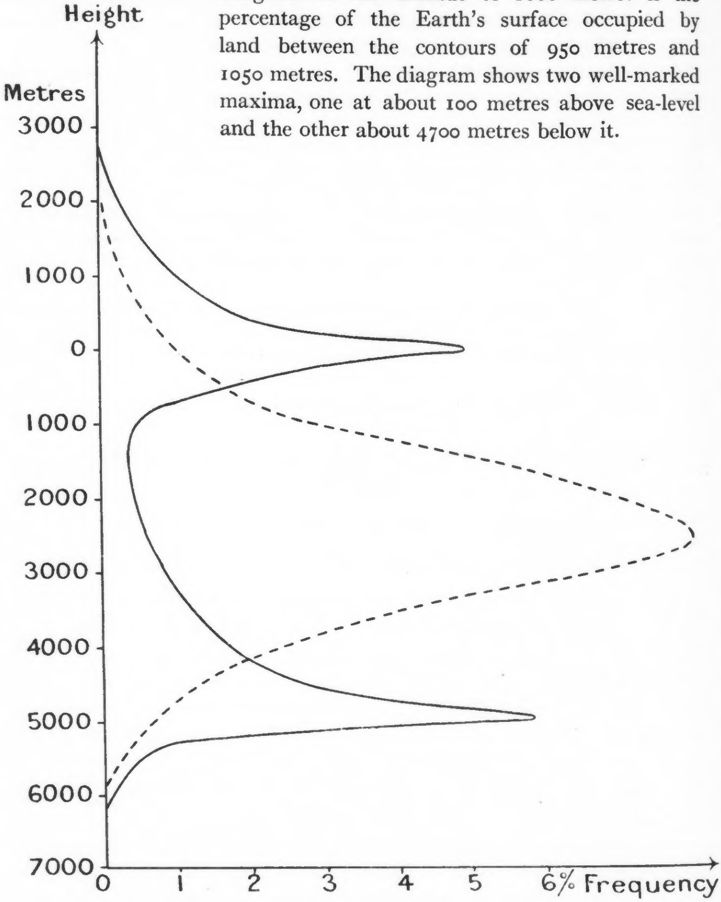

Fig. 1.—Frequency Curve of Height-distribution on the Earth.

Wegener concludes that two distinct surfaces standing at these two altitudes must have been involved in the subsequent movements that produced the present inequalities. He assumes that these surfaces were originally level—or, more strictly, equipotential—and that they were the surfaces of the Sal and the Sima respectively. He points out that if originally there were only one such level, the deformation of that level could not produce two maxima unless physical causes were present which would give a preference to two particular altitudes. He proceeds, "Since this is not the case, the frequency must be regulated according to Gauss's law of errors." The reasoning seems plausible, but he should have said, "Since, so far as I know, this is not the case"; and, taking this in conjunction with the rest of the sentence, we find that we are led to the remarkable principle: If we do not know the law, the law must be the law of errors; or, in more precise language applied to this particular instance, if we do not know the physical law that has determined the deviations from an original level, their frequencies must conform with the normal law of errors. And so the actual heights must be influenced by the extent of our knowledge.

But Wegener's world is not an ordinary one. In his diagram he draws a broken line, and this, he says, follows approximately the course of the law of errors, according to which the frequencies would be regulated if one level only had been involved in the subsequent movements. The frequencies in the diagram are expressed as percentages, and in the ordinary world would add up to 100. Wegener's line gives a total of about 200.*

In reality, if it is only a single level that has been deformed, it is improbable that the resulting altitudes will conform with the normal law of errors. The crust of the earth is not so constituted that each point can move independently of the rest, and the movements therefore are not analogous to the errors in a series of independent observations. According to the geological evidence the greater movements, which have most influence on the frequencies, are of a broad and widespread character. They are the epeirogenic movements of geologists, and their general effect is to throw the surface into broad undulations, the altitude of which, though it may be great, is small compared with their lateral extent. This is indicated by the fact that over the larger part of the Earth the sedimentary beds in general, and especially the Mesozoic and Tertiary deposits, are but little disturbed. Upon these broader movements are superimposed the more intense but more local movements which are distinguished as orogenic or mountain-building movements.

Mr. G. V. Douglas † has indeed pointed out that if we start with a level, or equipotential, surface, and suppose it affected by movements of the types referred to, the resulting altitudes will necessarily give a frequency curve showing two maxima, and similar in all essential respects to the actual frequency curve. If a level surface be thrown into a broad anticline and syncline, as shown in Fig. 2, it is evident at once that two contours at a vertical interval of 100 metres will enclose a larger area at the crest of the anticline and at the trough of the syncline than in any other position. The frequency curve, in this case, will show a maximum at each extremity and a minimum at the original level. If smaller undulations are superimposed upon the larger ones the resulting curve will approximate to the real one.

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* A mere inspection of the diagram is enough to show that the two curves cannot both be correct, for the areas contained between them and the vertical axis should be equal.

† His paper will appear shortly in the *Geological Magazine*, and he allows me to refer to it here.

It is unnecessary to say more here. It is sufficient that the actual frequency curve is perfectly consistent with ordinary geological conceptions and does not require the original existence of the two distinct surfaces postulated by Wegener.

Wegener imagines that at the close of the Carboniferous period the Sal formed one continuous patch covering about half the globe, and the Sima covered the rest. It should be remembered that this was not the distribution of land and sea, for the sea spread over a portion of the Sal.

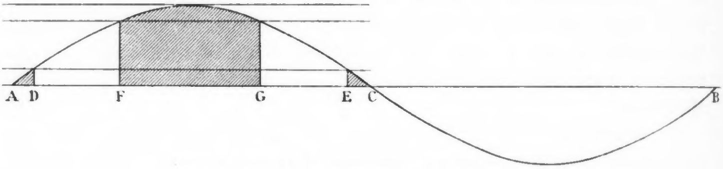

Fig. 2.—Area of Syncline enclosed by Contours.

He professes that he has taken the forms of the existing land-masses, including their continental shelves: he has modified the present forms by unfolding the mountain ranges which have been raised since the Carboniferous period; and he finds that the different patches can then be fitted together into one continuous whole, like the pieces of a puzzle. Africa and South America are brought into contact and united with Antarctica, to which Australia also is joined. A greatly elongated Indian Peninsula is wedged between Africa and Australia, and here also lie Madagascar and Ceylon. In the north, Asia, Greenland, the north of North America, and the north of Europe, with all the neighbouring islands, are brought together. But a gap remains between southern Europe and northern Africa on the one hand and America on the other.

A point of considerable importance arises here. He says that he has taken the edge of the continental shelf as the boundary between Sal and Sima. But, according to his own conception, the Sal floats in the Sima and the surface of the Sima is the deep-sea plain. The boundary between Sal and Sima should therefore be drawn at the foot of the continental slope, where the continental masses begin to rise from the deep-sea plain; and the shapes that he has fitted together are not the right ones. Wegener indeed allows that the foot of the slope is the present boundary of the Sal, but it was not the original boundary. Under their own weight the masses of Sal have bulged outwards, the maximum bulge being at the Sima level. Why the edge of the shelf should preserve the original outline he does not explain.

Assuming, however, that the original forms are really defined by the present edge of the continental shelf, it is evident that Wegener has given free play to his imagination. In following the edge he has allowed himself a very considerable amount of latitude. Where it was necessary in order to make them fit, he has not hesitated to distort the shapes of the masses. And few geologists who are familiar with mountain structures will attach much value to Wegener's estimates of the effect of Post-Carboniferous folding.

In order to obtain an idea of the amount of distortion involved in Wegener's reconstruction a very simple plan is sufficient. Upon a globe stretch a white thread through Cape Town and the head of the Gulf of Guinea. It will pass nearly through Valentia in Ireland. Mark these points upon the thread and stretch it along the east coast of South America, placing the mark for the Gulf of Guinea off Cape St. Roque. The mark for Valentia will now lie some 300 or 400 miles west of Spain—incidentally, you will probably be convinced that neither the coasts nor the continental shelves of Africa and South America fit so well as you had supposed. In order to complete Wegener's reconstruction it is now necessary to pull the north of North America right across the Atlantic until Newfoundland nearly reaches the Valentia mark.

It is easy to fit the pieces of a puzzle together if you distort their shapes, but when you have done so, your success is no proof that you have placed them in their original positions. It is not even a proof that the pieces belong to the same puzzle, or that all the pieces are present.

Even in the case of Africa and South America Wegener finds it necessary to ignore the Abrolhos Bank, and dismisses it in rather contemptuous fashion. It is a mass of molten Sal which has risen from beneath South America as that continent was pushed towards the west. But he gives no evidence whatever in support of this view. It is not the Abrolhos Bank that ought to be ignored. Wegener, indeed, goes so far as to compare the fit of Africa and South America with the fit of the two pieces of a visiting-card which has been torn in two. There is no difficulty in fitting the edges of any torn cards together if you are allowed to cut off pieces and stick them on the under surface.

If Wegener's hypothesis rested solely on the evidence of fitting that he brings forward it might well be treated as he treats the Abrolhos Bank. But there is more to be said for it than this.

In the Indian Peninsula the oldest fossiliferous group of rocks is known as the Gondwana System. Its deposition began about the close of the Carboniferous period and was continued to the end of the Jurassic period or even into the Cretaceous. The deposits are of terrestrial origin and contain remains of plants and of reptiles. The flora of the lower part includes, amongst other forms, the genus *Glossopteris* and is commonly called the *Glossopteris* flora. It is very distinct from the contemporaneous flora of north-western Europe.

There is a similar series of terrestrial deposits in South Africa, known as the Karroo System, and another in Brazil which is called the Santa Catharina System. Both of these contain the *Glossopteris* flora and remains of reptiles.

The *Glossopteris* flora is found in Australia, where, however, the terrestrial deposits are interstratified with marine beds. It occurs also in the Falkland Islands, in the Antarctic continent, and in other parts of South America besides Brazil.

In Wegener's reconstruction all these areas are brought together and it is easy to understand why they should have a common flora and why that flora should be different from the flora of the distant Europe.

But the statement is not yet complete. The *Glossopteris* flora is found in Kashmir, in north-western Afghanistan and north-eastern Persia, in Tonquin and in northern Russia—also, though without *Glossopteris* itself, in Siberia. In Wegener's reconstruction all these areas lie far from the masses that he has grouped together in the south.

The Russian deposits * are especially interesting. Not only do they contain representatives of the *Glossopteris* flora, but they include reptiles of the same type as those which are found in South Africa, and several species of freshwater shells which are identical with those in the South African beds. Wegener's explanation has not by any means simplified the problem of the distribution of the *Glossopteris* flora and of the fauna associated with it.

But in the areas which he has grouped together—India, South Africa, South America, and Australia—there are interesting similarities in the character of the deposits themselves. In all of them the system containing the *Glossopteris* flora begins with a boulder bed, which is now universally admitted to be of glacial origin. As these glacial deposits are situated at the present time it is very difficult to account for them. They are scattered over a wide extent of the Earth's surface. Even if we admit movement of the pole, on the most favourable supposition the ice must have spread much farther towards the equator than the ice-sheets of the Pleistocene Glacial period ever did. Nor is it possible to invoke the aid of icebergs, for the associated deposits, except in the case of Australia, are all of terrestrial origin. With Wegener's reconstruction these difficulties disappear. The areas are grouped together and the pole may be placed conveniently in the middle of the mass. There is, indeed, another minor difficulty. In South Africa the ice moved in a southerly direction, and a pole which would suit this movement would not be favourably placed for the whole mass. But it is not a serious objection, for in the Pleistocene Glacial period there were several centres of dispersion.

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* They were discovered by Amalitsky. Several of his papers are in Russian, but there is a summary of one of the most important in the *Geological Magazine* for 1901, p. 231, and a paper by Amalitsky himself in *Quart. Journ. Geol. Soc.*, vol. li. (1895), p. 337.

Again, however, the statement is not complete. The boulder beds of this period are not limited to the areas that Wegener has grouped together. There is a boulder bed in the Salt Range which appears to be of the same age as the Talchir boulder bed of the Indian Peninsula. It is associated with marine deposits. In north-western Afghanistan Griesbach * found a boulder bed which he recognized as similar to the Talchir boulder bed, and he was thoroughly familiar with the Talchir series. The beds overlying the boulder bed are partly marine and partly terrestrial, and in the latter he found several of the characteristic plants of the *Glossopteris* flora.

Wegener has altered his views about the positions of the poles, and in the third edition of his book he makes the South Pole wander freely over the masses of Sal. He gives a map showing its movements during the period from the Lower Carboniferous to the Upper Permian, an interval which would include the deposition of the boulder beds in question. At its nearest the South Pole lay some 60 or 65 degrees from north-western Afghanistan. That is to say, the boulder bed which occurs there must have been deposited within 30 degrees of the equator of the period. And it cannot have been laid down at a great elevation, for the beds that conformably follow it include both marine and terrestrial deposits. Wegener's ideas have not very greatly reduced the area that must have been affected by the ice of the Permo-Carboniferous Glacial period.

It is usually supposed that the boulder beds of India and the southern hemisphere, with which we have been dealing, were laid down during the interval between our own incomplete Carboniferous and incomplete Permian, but exact correlation is hardly possible. It is an interesting fact that both in England and in Germany the Permian System includes a remarkable series of breccias and conglomerates presenting very peculiar characters. In our own island the fragments are not uncommonly striated and some of them look like glaciated boulders. Ramsay, indeed, attributed them to glacial action and believed that there was a Permian glacial period; but this view has not been generally accepted. It is usually supposed that the deposits were screes formed on the flanks of upland regions, and that the scratches were caused by movement of the material in the screes before consolidation and movement along faults after consolidation. If to this we were to add the possible existence of local glaciers in those upland regions the whole of the phenomena would certainly be accounted for. It is clear that the German breccias were screes, for they occur on the borders of masses of ancient rock which were uplands then and are uplands now. The material spreads out some distance over the plains, perhaps carried down by occasional torrential rains or perhaps over snow-slopes in the winter.

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* Records *Geol. Surv. India*, vol. xviii. (1885), p. 62; see also vol. xix. (1886), pp. 239, 242.

The formation of these extensive scree deposits indicates that the climate of the period was extreme. Either there were great changes of temperature or the temperature fluctuated about the freezing-point so that frost action was excessive. So far as it is possible to judge from Wegener's map, the region must have lain about the equator.

There is another line of evidence that Wegener puts forward, but as I have already dealt with it elsewhere (*Geological Magazine*, 1922, p. 338), my treatment must be brief. He says, in effect, that when the patches of Sal are fitted as he has fitted them, the geological structures are carried across the junctions. If this were really so it would indeed be a proof that his reconstruction was correct. But his facts are not.

There are five geological features, according to him, which occur on the two sides of the Atlantic and which are re-united when the patches of Sal are fitted together.

The strike of the ancient gneiss of the Hebrides and northern Scotland becomes, he says, continuous with that of the gneiss of Labrador. The former, according to him, now runs from north-east to south-west, the latter from east to west. But according to the Geological Survey of Scotland the prevalent direction in Scotland is W.N.W.–E.S.E. or east to west. If Wegener's direction fits the other side the real direction does not.

The Caledonian folds of Scotland and Ireland, he says, become continuous with those of Newfoundland. It has generally been held that there are no Caledonian folds in North America. Termier (*C. R. Acad. Sci. Paris*, 157 (1913), p. 621) indeed has suggested that the Newfoundland folds form a delayed ramification of the Caledonian System, but in any case they are of considerably later date than the Scottish folds. With the distance between them that now exists Termier's view is intelligible, but if they were in actual contact it is not. The earlier Scottish folding, in spite of its great intensity, must have ended abruptly at the line where separation was to take place ages afterwards, and on the other side of the line the commencement of the later Newfoundland folds must have been equally abrupt. Both sets of folds are of Palæozoic age; the separation took place, according to Wegener, after the Glacial Period.

Farther south the Armorican folds of Europe, in Wegener's reconstruction, are continued by the Appalachian folds of North America, and no objection can be raised on the score of age. But a single coincidence of this sort has no value as evidence, for Wegener has adopted the simple plan of bending North America so that the ends of the two sections meet and the folds fall into line.

In Africa, according to Wegener, the ancient gneiss foundation shows a sudden change of strike at the head of the Gulf of Guinea, and in South America there is a similar sudden change at Cape St. Roque. When the two continents are brought together the two different strikes and the line of separation between them become continuous. But in bringing about this coincidence he gives to the gneiss north of the Gulf of Guinea a north-east to south-west strike, and this is very far from the truth. The strikes recorded by Falconer in Nigeria are for the most part nearly north and south, and a reference to the third volume of Suess's 'Das Antlitz der Erde' will show that over a still larger part of the area, in which Wegener imagines the strike to be from north-east to south-west, the actual observations indicate that the prevalent direction is from north to south. And thus in this case also Wegener's coincidence is imaginary.*

In South Africa a folded mountain range runs from east to west. In Buenos Aires a folded range belonging to the same period has been described. When the two continents are brought together one range becomes, according to Wegener, the continuation of the other and the folds that formed them are continuous. But before they reach the western coast the South African folds, and the range that they have formed, turn to the north and run roughly parallel to the western coast. If the two areas were separated as widely as they are at present, there would be no difficulty in a connection, but if they were in contact the north-to-south folds of the western end of the South African system must have doubled sharply back upon themselves and then turned at right angles in order to join the folds of Buenos Aires. Of this bending there is no sign, and the more closely the two continents fit the smaller is the space for these extraordinary evolutions.

In his third edition Wegener suggests that the northward deviation of the African folds was brought about during the separation of the masses. Presumably there was a shearing movement, and South America slid northwards along the coast of Africa. At that time South America fitted closely into the Gulf of Guinea, and it is difficult to see how any northward movement could take place.

From this brief account it will be clear that the geological features of the two sides of the Atlantic do not unite in the way that Wegener imagines, and if the continental masses ever were continuous they were not fitted as Wegener has fitted them.

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* In this connection it is interesting to compare the tectonic maps of South America by Keidel and of West Africa by Chudeau. The former is reproduced in Wegener's third edition, the latter will be found in *Bull. Soc. Géol. France*, 1918, p. 64. Both rest on very imperfect data, and in particular the age of the folds shown is very uncertain. Chudeau, on very slender evidence, draws a line of syntaxis through the corner of the Gulf of Guinea. Keidel makes a change of strike about Cape St. Roque, but in his map it is clearly not a syntaxis. If Africa and South America are united, Chudeau's folds north of the Gulf of Guinea will not join with Keidel's folds. A piece of the puzzle is missing.

Before the paper the **PRESIDENT** (the **EARL OF RONALDSHAY**) said: The paper this afternoon is to be given to us by Mr. Philip Lake, and the subject is "Wegener's Hypothesis of Continental Drift." It is an hypothesis which, I gather, has raised a considerable amount of controversy, and certainly one upon which there is a wide difference of opinion amongst those who have studied the question. It should be interesting, therefore, to us this afternoon to have the views of one who has made a study of this question and who certainly has come to conclusions which differ somewhat widely from those of Professor Wegener. I will ask Mr. Lake to give us his paper.

Mr. Lake then read the paper printed above, and a discussion followed.

Mr. G. W. **LAMPLUGH**: It may seem surprising that we should seriously discuss a theory which is so vulnerable in almost every statement as this of Wegener's. Yet Wegener's hypothesis is of real interest to geologists, because it has struck an idea that has been floating in their minds for a long time. Mr. Lake has touched many basal points at which the theory will not hold water, and other flaws present themselves to the specialist in various particulars. But the underlying idea that the continents may not be fixed has in its favour certain facts which give every geologist a predilection towards it in spite of Wegener's failure to prove it.

First, there is the fact of the big overthrusts that we know of in many parts of the world, by which great slices of the Earth's crust have moved for long distances, so that rocks have been brought over one another that were once far apart, sometimes as much perhaps as 100 kilometres. The movement of these big sheets of rock is difficult to explain, but it may be thought of as an expression at the surface, under local conditions, of some slow irresistible creep of the continental masses.

Then there is another set of facts to which Mr. Lake has referred, arising from the very curious old glacial beds which in many parts of the world reach into the Tropics. There seems no other explanation than that these deposits have been due to ice-sheets; yet we cannot imagine how ice-sheets could exist if the land was in the latitudes where it now lies. It would be very convenient to allow that the land has crept down from latitudes where glaciation was possible. In the same way with the complementary difficulty, that when we go up into the Arctic—to Spitsbergen, or any other parts of the northern regions that have been studied—we find stratified rocks covering a large portion of the Earth's history, yet containing no indication of glaciation until recent times. Here again a long creep of the land-masses would help us.

Again, there are many curious circumstances in the distribution of life-forms, not only of the *Glossopteris* flora and fauna, but others also of later times that would be explicable by movement of the land-masses. And further, there is the accumulating evidence that at a certain depth the interior of the Earth is not so rigid but that there might be some slipping of the crust over it.

For these, among other reasons, the geologist has been quite ready to take Wegener's views into consideration, and has, in the main, been rather grateful to him for bringing together these ideas so definitely and for setting himself up to be shot at, as it were. We are discussing his hypothesis seriously because we should like him to be right, and yet I am afraid we have to conclude, as Mr. Lake has done, that in essential points he is wrong. But the underlying idea may yet bear better fruit.

Mr. R. D. **OLDHAM**: I had no intention of joining in the discussion of Dr. Lake's paper, but as I have been called on I may perhaps remark on two points which lie somewhat outside of it. The first is that I have been surprised here and in other discussions on the Wegener's theory to notice the unanimity with which it is regarded as a novel idea. That, I think, is not the case at all, for I can remember that when I started as a geologist the idea was not unknown that a good deal of geological evidence was, at any rate, not inconsistent with the notion that the continents have not always occupied the positions on the surface of the globe which they do now. But also I can remember very well that in those days it was unsafe for any one to advocate an idea of that sort. The physicists, who before that had forced on us the notion of a fiery globe with molten interior and thin crust on it, had gone round and insisted on a solid heated sphere, and they would allow us to appeal to nothing, as the cause for various structures and changes that we knew in geology, but the slow cooling and contraction of this solid globe, and any notion of the shifting of continents was incompatible with that theory. Those ideas held the ground so strongly that it was more than any man who valued his reputation for scientific sanity ought to venture on to advocate anything like this theory that Wegener has nowadays been able to put forward.

But there was one man, even then, who did quite formally propose and maintain something which, to my mind, is almost identical with the Wegener hypothesis. That was Osmond Fisher, a man who in his time was a scientific Ishmaelite. He had advocated notions that in those days were rejected unanimously, but had he lived till now he would have seen that under an altered nomenclature we have come round to something not very unlike what he maintained. We may not speak of the fluid interior of the globe or connection currents, but we may safely talk of an asthenosphere and of under-tow, which are not so very different in effect. Now, in his ' Physics of the Earth's Crust,' and especially in the second edition, he quite definitely advocates the idea that the continents have moved in the past relative to each other. That has not been entirely ignored in recent discussions, but he has been regarded as merely supposing that America had separated from Europe and Africa, and drifted towards the cavity, in the region of the Pacific, left by the departure of the Moon into space ; but if you read Fisher's book carefully you will find that he suggested a great deal more than that. He recognized the general principle that the continents were capable of drifting about in the Earth, and that they have not through all time maintained the same position relative to each other and to the Pole and Equator that they now do.

And there is another point, too, about Fisher's old work which anticipated Wegener's. He did not speak of Sal or Sial and Sima, because those words had not been invented in his days, but he certainly did maintain that the physical observations on the Earth implied that the ocean beds were composed of a different material from that of the continental masses, and of a denser and more basic nature, that is to say the Sima ; whereas the continents were composed of a less dense and more acid material, the Sial or Sal. Fisher also anticipated Wegener in recognizing the general resemblance between the outlines of Europe and Africa on the one side of the Atlantic, and North and South America on the other. So that I think that when we are discussing Wegener and his notions as novelties it is as well to remember that we had a fellow-countryman who, years before Wegener started to speculate, had arrived at conclusions very like those which he has advocated.

The second point which I wished to mention was this : I should like to express a hope that in this discussion of Wegener's theory we will remember that the important question is not whether Wegener is right or wrong in his specific conclusions, but whether the continental masses have throughout all times maintained their present position relative to each other and to the poles; in other words, it is the doctrine of the permanence of oceans and continents.

There is a good deal in geology, as I have said before, which is not quite consistent with this doctrine, and one thing which, I think, is quite definitely against it, and that is the climatic conditions of the Upper Carboniferous period. It so happens that this particular geological period is represented, in nearly every quarter of the globe, by dry-land continental deposits, and consequently have some indications of the nature of the climate in those days. When these are viewed as a whole, not merely considering some of the regions but taking them all into comparison, we arrive at a condition which cannot be satisfied as long as those continents maintain the same relative distance from each other, whatever their relative position to the Poles and Equator may have been, but this is too large a subject to go into in detail.

Mr. F. DEBENHAM : I am afraid I shall be somewhat of an echo to Mr. Lake and Mr. Lamplugh, but there are one or two points to which I should like to refer. Like most of us, I think, I have been very much impressed with Mr. Lake's clear exposition of the evidence as given by Wegener ; it was rather more than an exposition, practically an explosion. He has exploded most of the geological evidence—all except one link, in fact. And I think on Wegener's statements, as he has made them, we must admit, as Mr. Lamplugh has said, that the theory is not holding enough water. But in justice to Wegener I think we ought to realize that he is an exceedingly bad advocate for his own theory. He has, for instance, gone to two most obscure parts of the Earth to find the strikes of foldings that are to fit his theory. Another proof that he brings forward in the book is what I think he calls a geodetic one—it is hardly that—a matter of a measurable movement of Greenland away from Europe, and that is one of the worst places in the world to expect accurate geodetic measurements. Any one who has dealt with that sort of work would be quite unwilling to admit that the accuracy of measurements over a few years would give anything like the value to the data which Wegener claims. He is not only a very bad advocate, but he changes his ground so peculiarly. You will remember that he takes extreme care to ensure an accurate fit between South America and Africa. He is very particular about that. He certainly strains his conscience, I think, and sacrifices a certain amount of truth in order to make those fit exactly ; whereas other continents do not fit anything like so well.

Now consider for a moment that peculiar diagram of the block of Sal and the mass of Sima in which it is floating which was shown on the sheet. We do not imagine it was exactly like that. We must imagine that there was some sort of merging of the lighter Sal into the Sima, and that there was a certain amount of similarity in their physical properties, a certain amount of common plasticity. The Sima may have been the more plastic, but still the Sal was also plastic. In fact, Wegener himself admits that the Sal continents have bulged to form the continental shelves.

Coming back to the fit of Africa and South America, or rather the separation, we are asked to imagine that the mass of South America has broken away from Africa, has displaced something like 2000 or 3000 superficial miles of Sima, and yet, as Wegener asks us to believe, has still retained the shape of the break exactly on that side and is an accurate fit. It is an instance of his bad advocacy that in that particular case he takes trouble to prove an exact fit, whereas if you follow out the geophysical consequences of it you must imagine that the Sal masses, or continental masses, would begin to change shape as soon as they broke away, and the last thing to be expected would be an accurate fit.

However, I believe we are all ready to be kind to the germ of the theory, in fact, most people are rather anxious that something of the sort should be proved, and we have to thank Professor Wegener for a great deal in bringing it forward and offering himself as a target for bullets. Geographers and geologists generally, I think, have looked with a short vision up to the present day at a good many of the problems. We imagine we know all about the ordinary process of the minor features of the Earth ; we have classifications for them, cycles of erosion and what not ; and if you present a geologist with a small area, he will tell you a great deal about it, most of which, I dare say, would be accepted. But larger areas have been somewhat neglected. Now, not for the first time perhaps, but for the first time boldly, Wegener has come forward with a theory which deals with the distribution of the continents in a bold way and offers himself for sacrifice ; and he is certainly getting it. So that in addition to thanking Mr. Lake for his very clear undermining of the theory, I think we certainly ought to thank Professor Wegener for offering himself for the explosion.

Dr. HAROLD JEFFREYS : I came here rather intending, if I said anything at all, to answer the physical arguments for the theory ; but none have been offered. My main complaint against the theory is that the physical causes that Wegener offers for the migrations of continents are ridiculously inadequate. They are something like a millionth, or less, of what is required to produce the shearing of the continents. A new suggestion has been brought forward by Professor Joly in *Nature* this week which, when you look into the numerical data, requires that a force equivalent to about the hundred millionth of a gramme weight per square centimetre should have been capable of moving the whole of the Asiatic and African Continents away from America. Wegener's suggested causes are of the same order of magnitude.

I think that there are probably two forces which are perfectly capable of producing large migrations of land and sea, if they are wanted. One is the change of rotation of the Earth. The Earth is gradually being led on account of tidal friction to rotate more and more slowly, and the rate at which this is going on at present is known. If it has been uniform, and if you take the estimates of the age of the oldest rocks given by Arthur Holmes and various others, you find that the Earth at the beginning of the Cambrian age must have rotated in something like twelve or fourteen of our present hours—an extremely great change. If the Earth rotated in half the time its equatorial bulge would be four times as big, so that one must not be surprised in geology if there is evidence that there has been a contraction round the Equator of several hundred miles, simply owing to the change in the Earth's rotation. This force is perfectly capable of splitting up continents as much as you want. Whether there has been a split of the kind that this would give is a matter for the geologist to settle.

The other force that I should like to mention is one that is indirectly suggested by a paper that Dr. Jeans published in the *Proceedings of the Royal Society* some years ago. It was suggested once by Dr. Jeans that the Earth was really more or less in the form of a pear, the Antarctic Continent being the stalk end of the pear, and that that had arisen on account of an instability of the spherical distribution of density inside the Earth. The idea was that if you started with a perfectly symmetrical Earth it would be possible for the heavy interior to move a little bit to one side and attract the ocean to that side, leaving a continental protrusion over the other side, and in that way you could account for nearly all the land being in one hemisphere. Jeans, however, discovered a mistake in his first method of attack and developed a new one. He then found that with any distribution of density, as long as it increases towards the centre of the Earth, the perfectly symmetrical state is stable. It would be quite impossible for a displacement of the centre towards one side to be produced of its own accord. Thus you cannot get an explanation of land and water hemispheres in this way.

But if you accept the new view you are left with another suggestion which may possibly be useful. At present most of the land is in one hemisphere. What Jeans has shown is that this distribution is unstable and, therefore, that the land-mass is tending to break up. Consequently, as long as the conditions are such that land once broken up can float about, there must be a tendency for the continents to split up and to redistribute themselves over the surface of the Earth so as to get as widely apart as possible. The force available for that is something like the pressure produced by a column of rock of the average height of a continent, which is quite a big pressure. It is quite possible that this may be of use for moving continents about, if you want them moved about.

Of course that leaves us still with the same difficulties as we had to start with. We still do not know why most of the land is in one hemisphere. Given that the land was originally all in one hemisphere, it seems to be possible to explain why it should proceed to spread about, but the only explanation so far offered of why the land could originally be all in one hemisphere seems to be Osmond Fisher's theory of the Pacific being the scar left when the moon was torn off the Earth. But it would be a very long business to go into that theory in detail. There is a great deal to be said for it and something to be said against it, and I must not take up any more of your time.

Dr. EVANS : Dr. Lake has made a damaging attack upon the very elaborate theory which Wegener has constructed. As Mr. Debenham has said, it is inconceivable that a very shallow thickness of Sial—I prefer to use that word, for as a result of time misspent on a classical education, I can't help confusing "Sal" with salt—extending over thousands of square miles could be moved horizontally without becoming distorted. Wegener attempts to prove that the continents have still the boundaries in which they originally united, and that the old idea of the foundering—the sinking, that is—of portions of what is now the ocean is entirely incorrect. I agree with what Dr. Lake has said about the absolute failure of Wegener to prove his case by demonstrating the continuation of the lines of mountains and foldings from one continent to another. Dr. Lake stated that Keidel's map of South America on which Wegener relies is based on very imperfect data. I can confirm that on the highest authority, because all the northern portion (with which Wegener is mainly concerned) is an exact copy line for line by photography or tracing from a map of my own which you will find in the *Quarterly Journal of the Geological Society* (vol. 62, p. 90, 1906). Dr. Keidel freely admitted it when I met him in 1913 in Toronto. It certainly was based on imperfect information, which was all that was then available.

Nevertheless, I think that there is evidence of the drifting of continents the one from the other, and that the distances which now separate South America from Africa are very much greater than was formerly the case. Dr. Baker, who has spent two years in the Falkland Islands on behalf of the Government in investigating their geology, has shown that the succession is, bed after bed, a repetition of that in the part of South Africa which is in the greatest proximity to the Falkland Islands. I find it more difficult to believe that there were exactly similar conditions spreading over the thousands of miles between South Africa and the Falkland Islands than that they have drifted apart. The similarity between the dolerite (diabase) intrusions in corresponding beds in South Africa and Brazil is also very remarkable. On the West Coast of Africa there is believed to be extensive faulting, resulting in a downward movement towards the west, which has probably submerged considerable tracts of land under the sea and at the same time has moved them to the westward. I have also been able to demonstrate that in the south-west of England there has been a general sliding away to the westward. Evidently in the Atlantic tract a state of tension has existed which must inevitably have been associated with a movement apart of its eastern and western shores.

Wegener believes that the Sima is capable of yielding to pressure and allowing the continents to drift through it. I cannot agree that this is possible, unless, as Dana pointed out many years ago, there has been in the neighbourhood of a continent a great amount of sedimentation which acts as a blanket on the sea bottom. That causes the older rocks below to rise in temperature and to lose their rigidity. This does not occur with the acid rocks of the Sial, which require a large amount of volatile fluxes to give them fluidity, but it is the case with the basic rocks or Sima, which lose their rigidity if the temperature is sufficiently raised, even without the presence of volatile constituents. Under those conditions there may be a yielding of the old sea bottom, a crumpling of the sedimentary rocks above it, and the production of new ranges of mountains. Something like that has been occurring all round the Pacific. I believe that there has been a general tendency to a movement from the centre of Africa towards the centre of the Pacific. But as a whole the Pacific Sima has acted not as a yielding area but as a resistant area, and it is only round the margin of the Pacific that folding has occurred and mountain ranges have been formed.

I think that, however open to criticism Dr. Wegener's views may be, he has done a service in directing attention to these important problems which I believe it will repay our time and trouble to study.

Mr. C. S. WRIGHT: There is only one thing I would like to say other than to congratulate Mr. Lake on his very clear exposition. I do not think the method of criticism is quite the right one. With a hypothesis that touches so many sciences, I think the only effective attack is a criticism of the hypothesis treated as a whole, not on a few points of detail. Moreover, the hypothesis is intended as an explanation of facts. As Mr. Lake points out, these include some fictions, but one must still compare this hypothesis with any others which are in the field in regard to their capacity for explaining these same facts. This is, I believe, the only fair method of criticizing Wegener's proposals.

The PRESIDENT: The impression left on my mind by the discussion is that geologists, as a whole, regret profoundly that Professor Wegener's hypothesis cannot be proved to be correct. If that statement had not been made so emphatically and so frequently this afternoon I should have been inclined to judge, from the vigour and vim of the speeches with which they destroyed the theory, that their satisfaction had lain in a somewhat different direction. What the general feeling no doubt is, is this: that some theory of this kind is required to explain facts which have long been known to geologists, and while they feel bound to condemn this particular hypothesis as being one which is not capable of meeting this long-felt want, they still hope that some other hypothesis of a kindred nature will be discovered which will satisfy their requirements.

I am sure we are all indebted to Mr. Lake for the paper which he has given us this afternoon. It certainly has raised an interesting question and has elicited interesting opinions. I am only expressing the feelings of the whole meeting when I venture to offer him, on your behalf, our most sincere thanks for the trouble which he has taken in drawing up his paper and coming and addressing us this afternoon.

EQUIPMENT FOR HIGH ALTITUDE MOUNTAINEERING, WITH SPECIAL REFERENCE TO CLIMBING MOUNT EVEREST

Captain G. Ingle Finch

Read at the Afternoon Meeting of the Society, 20 *November* 1922.

IN climbing Mount Everest, high altitude may be said to begin at 22,000 feet when the North Col is approached from the east. Although there is no sharp line of demarcation between what constitutes low and high altitudes, I select the above figure for the following reasons: Firstly, all the strongest and most physically fit members of the Expedition are of the opinion that at 21,000 feet (Camp No. 3) one's physical functions are practically unimpaired, and good sleep and recuperation from fatigue are possible; but at 23,000 feet (North Col), owing to the altitude, one's physical functions are impaired; sleep becomes fitful, in some individuals the appetite falls off, and there is a general loss of physical fitness. The conclusion may therefore be drawn that in the case of the most favoured individuals acclimatization to altitude ceases between 21,000 and 23,000 feet. Secondly, up to a height of 22,000 feet, snow and ice conditions approximate very closely to summer conditions in the Alps; above 22,000 feet, however, the state of the snow resembles that met with in mid-winter in the Alps. This high-altitude zone may be further divided into two sub-zones—the first from 22,000 feet (foot of the steep snow and ice slopes leading up to the North Col) to 23,000 feet, and the second from 23,000 feet onwards. The first zone is protected by the North Col from the prevailing westerly wind, whereas the second is fully exposed.

Equipment for First Zone.—Oxygen should be used from the foot of the North Col slopes onwards. No useful purpose is served by tiring oneself through not using oxygen, when, as we have seen, full recovery from fatigue is no longer possible at 23,000 feet.

Clothing somewhat warmer than that used in the Alps in summer is quite sufficient. A solar topee is advisable as protection against the sun, and Crookes' glasses of smoke-blue colour afford complete protection from glare without causing eyestrain and subsequent headache. It is also advisable to wear a veil or similar protection, and not to expose the hands to the rays of the sun. Sunburn is invariably followed by a condition of feverishness which cannot but impair one's fitness.